

Ecological consequences of elevated salinity in the Sacramento–San Joaquin Delta

Wim J. Kimmerer

Final Selection Panel Review

Proposal Title

#0105: Ecological consequences of elevated salinity in the Sacramento–San Joaquin Delta

Funding:

Fund with future funds

Amount: \$550,000

The final Selection Panel agreed with its original recommendation on the merits of this proposal. Due to the recent reduction in funds available for the Science Program's 2004 PSP, the Selection Panel has been forced to place this proposal in the Fund with Future Funds category. This decision was based solely on the current programmatic priorities of CALFED and the current level of available funds for purposes of supporting research efforts of this nature. This decision was not a reflection of the technical merit of this proposal.

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Public Comments

No public comments were received for this proposal.

Initial Selection Panel Review

Proposal Title

#0105: Ecological consequences of elevated salinity in the Sacramento–San Joaquin Delta

Funding:

Fund in part

Amount: \$550,000

Initial Selection Panel (Primary) Review

Topic Areas

- Life Cycle Models And Population Biology Of Key Species
- Environmental Influences On Key Species And Ecosystems
- Relative Stresses On Key Fish Species
- Direct And Indirect Effects Of Diversions On At-risk Species
- Processes Controlling Delta Water Quality
- Implications Of Future Change On Regional Hydrology, Water Operations, And Environmental Processes

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

This proposed project would improve the understanding of potential effects of increased salinity in the western Sacramento–San Joaquin Delta. Such an increase could result from prolonged drought, levee failures, or direct manipulation. The proposed project could help agencies prepare for and address consequences of such a salinity increase, or could identify salinity manipulation as a potential tool for restoration of the Delta and recovery of at-risk species.

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as

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Initial Selection Panel Review

much science per dollar as is reasonably possible. With this commitment in mind, can the proposed budget be streamlined? If so, please recommend and clearly justify a new budget total in the space provided.

The Technical Synthesis Panel remarked that some aspects of the budget should be better justified. One technical reviewer identified several apparent discrepancies in the calculation of the budget. If this project is to be funded, then additional justification for aspects highlighted by the TSP and the discrepancies identified by the technical reviewer should be addressed during the contracting process. In addition, this proposal could be considered for partial funding, focusing on those aspects of the proposal that collect basic salinity tolerance information on key species for which such information is apparently lacking. If such a focus is adopted, then portions of tasks 2, 3, and 5 should be funded. Funding portions of these tasks would result in a total budget of no more than \$552,058 (the sum of the total budgets associated with tasks 2, 3, and 5; although concerns about discrepancies identified above remain to be addressed).

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

The proposed project should yield information that managers will need sometime in the future, and that may prove useful for recovery of at-risk species. Salinities are likely to increase in the Delta as a result of drought or levee failure. In addition, risks to Delta water supplies associated with drought and levee failure, and potential benefits to invasive species control and recovery of at-risk species, suggest that managers should consider changes to Delta infrastructure to prepare for or actively manage increases in Delta salinity. The proposed project would provide basic information necessary to increase our understanding of the potential effects of salinity increases in the Delta.

Selection Panel (Discussion) Review

fund this amount: \$550,000

note:

fund in part

This study would look at salinity in the delta, which they argue will at some point increase for a variety of reasons. This proposal would be a potentially valuable step towards preparing for future change: What would the environmental consequences of increasing salinity in the Delta? This work could be a 'foot in the door' for ecological planning, and an opportunity to take a 5-50 year view scientifically.

The Panel was divided on this proposal. All felt that this question is a critical one for the future, and some felt that work should be started immediately. This work would link directly to management and infrastructure decisions, including a developing levee failure strategy. It is at least as timely as looking at climate change, but with more dramatic consequences.

Other panel members felt that it was not an urgent issue at this time, particularly given technical issues with this particular proposal. The Panel felt that it would be difficult to make predictions from the reductionist lab experiments proposed here, and recommend greater emphasis on testing effects of fluctuating salinities.

If funded, the proposal should be reshaped. It was recommended that the proposers eliminate efforts associated with die-backs of *Egeria* in conjunction with existing control efforts of the Department of Boating and Waterways. The proposal should focus on identifying salinity tolerance ranges for *Egeria*, *P. amurensis*, largemouth bass, and redear sunfish; and on other measures of response to salinities for these species. Portions of tasks 2, 3, and 5 should be funded. Tasks 1, 4, and 6 are not recommended for funding at this time.

Panel Ranking: Fund with major modifications.

Collaboration Panel Review

Proposal Title

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Final Panel Rating
superior

Collaboration Panel (Primary) Review

Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

superior

There are six distinct tasks with associated hypotheses outlined in this proposal (p. 10 and 11). The project clearly describes how they are inter-related, uncertainties associated with them (Table 1a), and the need for the interdisciplinary, collaborative project as designed to address this complex problem.

Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

superior

Figures 1, 2, 3 and 5 conceptually present the interactions between the subprojects. These conceptual models and the accompanying text describe how the project components fit together (interactions between salinity, vegetation, and local hydrodynamics). On page 11 and 22 of the proposal, the project

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Collaboration Panel Review

proponents discuss the project strategy and the possible issue of potential differences in the stages of the subprojects. The proposal also includes explicit plans for analyzing and interpreting the data in an integrated manner. A model of the response of Delta habitats to increased salinity will be used to synthesize the experimental results (p.11-12). In addition, a series of workshops and meetings are planned to promote this "third phase" of synthesis (Table 3, page 30).

Project Management:

Is it clear who will be performing management tasks and administration of the project? Are there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

above average

Although it appears from the text that the PI (p.22) will be responsible for overall project management, another person (Kenneth Paap) is listed in the personnel section as the primary person responsible for oversight of grant operation, management, and reporting. This person is not listed anywhere else in the proposal - mistake? There is no task associated with project management and coordination, nor any explicit funding in the budget. There is funding in the budget in several places for "collaborative research". Figure 6 indicates how information will flow in the project and the project organization, and project management meetings are described in the text (twice per year).

Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

superior

The PI appears to have management experience leading collaborative teams based on past work experience, proposals funded, etc, but this is not explicitly stated in the

Collaboration Panel Review

proposal. Table 2 outlines all the tasks and the key personnel assigned to each task, including deliverables. The skills of the team appear to be complementary and are discussed in detail (p.20).

Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

above average

Reporting of progress is described in the proposal (p.21) with presentations, articles, and reports. There is no plan for a broader outreach program.

Additional Comments:

Collaboration Panel (Discussion) Review

Primary rated collaboration and integration as superior. Six tasks described with hypotheses, interrelated, uncertainties identified, needs identified with a series of workshops and meetings to promote project synthesis of information. Project management was average because no specific tasks thoroughly discussed; budget was associated with planning or meeting; information flow identified only by figure and description. Communication of results was above average; that is, progress reports, standard articles, workshops, but no broader outreach mentioned.

Secondary rated the proposal superior. The PI's reputation and achievements seemed to favoratively influenced the review panel. On pg 22 the unique figure showing interactions between tasks was a great example. Key leaders and providers of the information were identified. Subtasks plans clearly defined. Models presented were good. Text does recognize problems and identifies solutions.

Collaboration Panel Review

Both reviewers as well as the entire panel judged the project superior.

Technical Synthesis Panel Review

Proposal Title

#0105: Ecological consequences of elevated salinity in the Sacramento–San Joaquin Delta

Final Panel Rating
above average

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The premise for this study is insightful and important: in a biotic community that evolved with highly varying salinity, the prospect of future changes and the need to restore such variability to reduce invasive species requires information on how such changes would affect an already highly altered community. This proposal brings together investigators with a large body of experience on various system components to address system-level effects of salinity change. The conceptual model is very thoroughly thought out from both biological and physical perspectives, and focuses the study on a major change in habitat structure and function expected with increased salinity - loss of *Egeria*. A critical part of the study is the salinity tolerance work in greenhouse mesocosms. Although it is acknowledged that salinities fluctuate throughout diel tidal cycles (p. 12), and that salinity tolerance varies with the level of variability (p. 14), only static salinity tests will be used (p. 12). There are a number of studies in the literature for macrophytes showing that the degree of acclimation before experiments, local genetic adaptation, and the magnitude and rate of change of salinity have important effects on apparent salinity tolerance (this literature is not well cited in the proposal). Although less work of this type has been done on invertebrates, similar principles undoubtedly apply. In particular, the life stage of

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Technical Synthesis Panel Review

the plant or animal has huge effects on salinity tolerance, such that adults may tolerate well salinities that would prevent survival of earlier stages. Thus, without preliminary data, it is very possible that static salinity tests will yield data of unknown relevance to field conditions, or to effects of salinity change on the realized distributions of the organisms. To me, more reliable and relevant data would be gained from transplant experiments in the field, where organisms of different life stages are exposed to natural daily cycles of salinity at different mean salinities. This approach is proposed for fish (p. 18), but does not receive adequate consideration for plants and invertebrates. No methods are given for measuring oxygen consumption of bivalves, and there are no publications indicating that the PIs have experience with such methods. Respirometry can be very tricky. Also, it is not explained how food availability for bivalves will be decided on or standardized in flow-through mesocosms (p. 16) - what food will be used, at what concentrations, and how will concentrations be controlled? These are not trivial issues, as they will have important impacts on measured variables such as feeding responses and growth rates. The publication record of several investigators, and notably that of the Lead PI, is excellent. I have no doubt that results of this study would become available in the primary literature in a very timely fashion. Although there are many shortcomings to methods proposed for various tasks, such problems are inevitable in a study of this scope. I feel that these investigators will gather much information relevant to CALFED, and will publish that information in insightful ways.

Additional Comments:

It is unclear whether hydroacoustic surveys will be sensitive enough to detect changes in the density and size distribution of small fish (p. 18). The budget for physical dynamics seems too high without better justification: \$22,500 for disposable lab supplies, \$53,750 for modeling computer system (an unreasonable cost without better explanation), and \$233 K in salaries. 100% support for Sereno for 2.5 years seems excessive. However, in total, the budget is reasonable

Technical Synthesis Panel Review

considering all that will be done. There is concern that if all proposals that Kimmerer has submitted were funded, he would be over-committed.

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Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Ecological Consequences of elevated salinity in the Sacramento-San Joaquin Delta

The panel indicated that the static salinity studies proposed would provide limited data on response to varying salinity, and the research design does not address life stages. The proposal will provide forward-looking data. The proposal takes a reductionist approach to a holistic question. The panel indicated that the proposal is unclear as to how the individual pieces will all be pulled together. The simulation modeling proposed was not clearly described. The panel discussion indicated that above average salinity is a very good proxy indicator, but it is not necessarily the causal factor for determining where an animal is found. The panel recommends using field plots to study the proposed problem rather than lab mesocosms because that would incorporate other variables that would vary with salinity.

Technical Review #1

proposal title: Ecological consequences of elevated salinity in the Sacramento–San Joaquin Delta

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>This proposal, being that it focuses on the ecosystem-landscape scale, has very ambitious goals; scaling up from the experimental-mesocosm scale to the field is always problematic. However, the proposal has met this challenge by the integration of experimental work, field work, and development of ecosystem and hydrodynamic models. The objectives and hypotheses are clearly stated and internally consistent. The authors are very clear that the project would be the initiation of a better understanding of potential changes to the Delta region. The authors do an excellent job justifying why the CALFED Bay-Delta program should start examining how the Sacramento-San Joaquin Delta region would change, ecologically, with increased salinity intrusion. The current situation is highly tenable, based on the argument put forth by the authors. However, based on the CALFED Bay-Delta's priorities and potential to actually use salinity as an adaptive management tool to control exotics, examining oligohaline or mesohaline salinity changes may or may not be an immediate scientific question. This reviewer is not familiar with the issues and timelines for some of the management problems discussed by the authors. However, a proactive program before a pilot study is conducted, seems warranted, particularly with many of the uncertainties that could have long-term consequences for down-stream estuarine organisms as well as human health.</p>
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Technical Review #1

Rating	very good
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Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The justification for the study is based on past events, where salt water intrusion has occurred into the Delta region. Also, the decadal time scale of the CALFED Bay-Delta program necessitates an examination of changing and highly variable climate environments, which would include drought events and increased salinity into the Delta. The authors are very familiar with the Delta ecosystem and the research that has been done both site specific, and on the organisms in questions, particularly the dominant exotic species which are a major concern. The conceptual model is clearly presented, and while it could not possibly encompass all the dynamics of the system, the authors seem to understand and highlight the most likely significant changes and potential cascading effects of increased salinity in the Delta. What is not as clear, is whether or not this conceptual model will be able to be incorporated into an ecosystem simulation model to be predictive of changes in the ecosystem with increasing salinity. This later type of model will probably be a next step, based on the results of this first series of research experiments.
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

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<p>Comments</p>	<p>It seems that the approach for each task in the proposal has been well thought out, however specific details of some of the methodology and coordination of the experiments to facilitate incorporation into a common model may be lacking. For example, the fish experiments will be run at 0, 2.5, 5.0, 7.5, and 10 ppt salinity raising salinity 1 ppt/day, while the plant experiments will be run at 0, 5, 10, and 15‰ with no information given for rate of salinity increase, which will be very important for defining threshold salinities. No where in the proposal do the authors talk about the expected rates of salinity increase, but perhaps this is an unknown at the present time. The integration of experimental, field, and modeling approaches for examining how ecosystems function and change with perturbation is an excellent approach. However, it is difficult to determine whether the spatial articulation of the ecosystem response to some of the field perturbations is adequate (mortality of Egeria beds), because at this time the authors do not know which sites or how many sites will be chosen (no figures or maps were provided in the proposal). Regardless of some of the uncertainties in the actual experiments and field sites, it seems that the data collected will be very valuable for managers of the Delta ecosystem, particularly the control of Egeria. While the current management with Cu is successful, it could be a major problem over the long-term, because of the enrichment of Cu in the ecosystem. As the salinity tolerance of Egeria, and many of the other exotic organisms in the system is not well understood, the experimental work proposed in this study will be useful to management.</p>
<p>Rating</p>	<p>excellent</p>

Technical Review #1

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	If the authors come together and more fully integrate their experiments, such that the final data could be incorporated into a predictive model, the likelihood of success is very high. The synthesis meetings should be up front, before experiments and field work is initiated, as well as at the end of the project. The scale of the project is ambitious, but the PIs on the proposal are very experienced and have conducted research across scales incorporating a high amount of complexity. This type of integration is applauded and is the only approach to understand ecosystem responses of this magnitude.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Monitoring is not proposed, but if a drought occurs within the time frame of the research, the team should have the flexibility and a plan to monitor the response of the system to increased salinity exposure under in situ conditions.
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	
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Technical Review #1

	The products will primarily be research results that will set the framework for understanding the ecosystem response to increased salinity in the Delta, a likely future scenario. The work will also identify potential negative effects of increased salinity, so if a pilot-scale project were planned, the potential consequences of such an action would be better understood. The PIs are also proposing a report to the CALFED Science Program summarizing results of the research and providing recommendations, based on this research. The final modeling product is not well defined and not listed in the Expected Products/Outcomes section of the proposal-this might have just been an oversight of the authors.
Rating	good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The PIs on the project are highly qualified, and include collaborations between academic and government researchers, which will assist the project's focus on management issues. The PIs have published widely in their respective fields related to their tasks on the project and should have no problem completing the research. The facilities available to the PIs are also excellent.
Rating	excellent

Technical Review #1

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Because of the multi-disciplinary approach and necessity to include multiple PIs, the proposal budget is high. However, for the tasks being proposed, the budget seems very reasonable, particularly for tasks 2-6; task 1 budget is high, but it covers a PI on soft money (100% in year 2). This is compensated for by the minimal support being requested by the other PIs.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	The proposal to examine the potential effects of salinity increases in the Sacramento-San Joaquin Delta region is excellent to very good. In this proposal, the PIs put forth an excellent argument for why it is important to initiate research to attain a better understanding of how the Delta may change with increased salinity intrusion. Their first priority, to understand the interaction of the physical aspects of the hydrology of the Delta region with salinity, and the dominant SAV Egeria, is on target. Because of the dominance of Egeria in the system, it will be very important to know the salinity tolerance of this species, and the cascading effects on the area if a major die-off was to occur. Understanding the fate of this organic matter and how it relates to the basic biogeochemical cycling in the sediment, as well as potential Hg toxicity, a major human health issue, is also an excellent research arena to examine. The larger response by the ecosystem in terms
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Technical Review #1

	<p>of food web changes in the system, infauna, epifauna, migratory fish, and autotrophs other than Egeria, will significantly increase the knowledge of CALFED managers on how salinity could affect the ecology of the Delta before any pilot projects are considered. While this is an ambitious proposal, and because of this fact, a lot of details and specific potential for integration into a comprehensive model for predictive purposes may be lacking, the research will provide a very important first step in understanding how these highly maintained fresh water ecological systems may change with increased salinity. The PIs, while several of them are more experienced than others, all seem highly qualified to accomplish the tasks proposed. The budget also seems very reasonable, based on the high number of personnel involved.</p>
Rating	excellent

Technical Review #2

proposal title: Ecological consequences of elevated salinity in the Sacramento–San Joaquin Delta

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The proposal describes a scientifically interesting and management-relevant study that should generate important information about the potential effects of increased salinity in the Sacramento-San Joaquin Delta. The goals, objectives and hypotheses are clearly stated and are logically linked in a well thought-out conceptual model.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The PIs propose that increased salinity (if of sufficient severity and duration) will cause a dieback of SAV, which will then result in altered hydrodynamics, changes in detritus, dissolved oxygen and sediment, and increased availability of contaminants. Both the direct and indirect effects of increased salinity should vary for mobile and sessile species. Furthermore, after short-term negative effects of salinity abate, salmonid habitat should increase.
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Technical Review #2

	The proposed study is well justified based on the likely susceptibility to environmental change of the various species considered, as well as the potential for future planned or unplanned increases in salinity. The conceptual model clearly links the physical aspects of the system with predicted biological and chemical responses. The broad suite of issues examined, from changes in hydrodynamics, to responses of a variety of organisms, to changes in contaminant availability is a particular strength of the proposal.
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	<p>The proposed suite of measurement, experiments and models is ambitious, but feasible given the expertise and track record of the PIs. By taking a comprehensive approach including modeling, field measurements and experiments, the study promises to generate interesting, important information even if some aspects of the conceptual model are disproved.</p> <p>More detail on replication, planned statistical approaches and number of treatments would have been helpful to better evaluate the adequacy of the proposed experiments.</p>
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

#0105: Ecological consequences of elevated salinity in the Sacramento–San Joa...

Technical Review #2

Comments	As indicated above, the proposed study is ambitious, but feasible with this group of PIs. The conceptual model is persuasive in providing evidence that the most important potential changes to the system are being examined. The scale of the project is appropriate.
Rating	excellent

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The proposed research should improve both the basic scientific understanding of the system and the ability to predict the effects of management-initiated and unplanned increases in salinity. The PIs describe a suite of products including peer-reviewed publications, presentations to CALFED, technical reports, and data access that will benefit a wide audience. They have wisely left the third year of the project for data analysis, synthesis, and integration. This, combined with the demonstrated publication records of the PIs, should ensure that both individual components and the project as a whole are analyzed and published.
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Technical Review #2

Rating	excellent
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Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The authors of this proposal represent a highly qualified team with a diversity of areas of expertise relevant to the proposed research. The senior PIs have impressive publication records. Kimmerer, in particular, has made substantial contributions to the understanding and management of the Sacramento-San Joaquin Delta and greater San Francisco Bay region. Required infrastructure appears to be available to accomplish the proposed study.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget appears appropriate for the proposed work. The involvement of graduate students is good, and the major allocation of funding to labor is appropriate. The return-for-investment should be high.
Rating	excellent

Technical Review #2

Overall

Provide a brief explanation of your summary rating.

Comments	This study brings together a strong research team to examine an interesting and management-relevant question. The persuasive conceptual model is tested logically and thoroughly through both modeling and experiments. A good plan is proposed for data analysis, integration and synthesis.
Rating	excellent

Technical Review #3

proposal title: Ecological consequences of elevated salinity in the Sacramento–San Joaquin Delta

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The overall goal of this very ambitious project is to provide the information (model) necessary to make decisions concerning large scale experiments on salt penetration into the Delta. Specific objectives, addressed through development of a conceptual model, are 1) to determine likely ecosystem responses, 2) to determine the magnitude and duration of salinity increase necessary for unambiguous results, and 3) to determine whether such an experiment would provide useful knowledge or cause harm. These objectives will be addressed by a series of well-defined hypotheses concerning hydrodynamics and transport, salinity impacts on native vegetation, non-native vegetation, non-native bivalves, epifauna, phytoplankton, non-native fish, and impacts on copper and mercury cycling. This topic appears to be important; the authors indicate that the Delta will be salty at some point in the future, either through drought, levee failure, redirection of water by canal, or deliberately to control exotic species. However, the authors do not impart a sense of urgency to any of these scenarios.
Rating	very good

Technical Review #3

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	This project is well-justified in that it addresses principles for restoration and management that have been adopted by CALFED. The authors have developed a conceptual model (Figure 1) describing two potential scenarios concerning the impacts of salinity intrusion, depending on flushing rate. Additional possible scenarios are not depicted. The conceptual model justifies the proposed experimental work on non-native SAV, non-native fish, non-native bivalves and contaminants. Phytoplankton work was not mentioned until page 10 and was not as well justified as work on the other groups. While non-native fish will be examined, native fish will not - is that because their salinity responses are already adequately known?
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	This project will meet the objectives through a series of very ambitious field and laboratory experiments and modeling efforts. Results of these experiments will certainly generate novel information and add to our base of knowledge concerning SAVs, bivalves, epifauna, contaminant cycling and invasive species, with at least 6 peer-reviewed manuscripts. At least one novel methodology will be used and described, that of quantitative sampling for SAV epifauna using a net with purse string. The project will be useful to
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Technical Review #3

	decision makers in that the overall goal is to provide information necessary to make decisions concerning large scale experiments on salt penetration into the Delta. There are a few questions concerning the methods. While some portions of the project description are in exhaustive detail (Contaminants), other portions are lacking in detail. Methods and frequency of sampling for velocity, turbidity, plant detritus, etc are not mentioned. Net photosynthesis methodology and sample size are not provided. How many replicates will be used in Task 2, Phase 2? No methodology is described or cited for the measurement of oxygen consumption of bivalves measured under a salinity series. What endpoint will be measured in the phytoplankton analyses? Task 4, Phase 1: How will food (phytoplankton, plant detritus) be kept in overflowing containers?
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	This large-scale project is very ambitious but, given the experience of the participants, is probably feasible. Likelihood for success is relatively high especially given that the authors concede from the start that one of the acceptable outcomes may be the decision "No further action: the response was too weak, or detection too difficult". Feasibility of Task 1 depends on investigating a sudden die-back of Egeria in conjunction with control efforts of the Department of Boating and Waterways. No information is provided as to whether this is an event that is sure to occur annually. Some of the individual experiments may prove not to be technically feasible. For example I don't believe that the authors will be able to determine specific growth rate (based on wet weight) of epifaunal organisms in 7 days; noise will be too
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	great. However, no sample size, example of taxonomic groups to be examined, or example body sizes are provided so I cannot criticize with certainty. Having said that some individual experiments may prove to be technically unfeasible, I'm sure the authors are capable of adapting.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	While much of this project will be modeling efforts or laboratory experiments, some monitoring will be conducted to calibrate the hydrodynamic models. Parameters to be measured before, during, and after a die-back of Egeria include velocity, turbidity, conductivity, temperature, depth, dissolved oxygen, and suspended particle size distribution. Methods, spatial distribution and temporal frequency of monitoring are not described.
Rating	good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	Products include the model (although not mentioned by the authors specifically as a product), presentations, articles in newsletters, reports, and at least 6 peer-reviewed manuscripts. These will include manuscripts on physical dynamics (Sereno et al.), vegetation response (Boyer et al.), infauna response (Thompson et al.), epifauna and plankton response (Kimmerer et al.), fish (Nobriga et al.), contaminants
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	(Marvin-DiPasquale et al.) and a final manuscript interpreting and synthesizing all information. In addition, participants will engage in outreach activities in collaboration with Educational Coordinators at their institutions.
Rating	excellent

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	Based on past performance of these authors, the project team is very well qualified to efficiently and effectively implement the proposed project. One concern, however, is that Kimmerer is over-committed. Although no "Current and Pending" is required, Kimmerer is lead PI on another CALFED grant and is participating in three other related proposals. Secondly, it is not clear whether any of the authors have experience conducting bivalve oxygen uptake studies. Infrastructure appears to be available, although no vehicles are mentioned.
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	As is expected of an ambitious project such as this, the budget is large. I find it difficult to comment on the reasonability or adequacy of the budget. First, there were mistakes in calculations in numerous places. Examples follow. Direct costs for some Tasks
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	<p>appear to be miscalculated. I can see how they could come up with a lower number, if some items are excluded from overhead charges, but I don't see how they come up with a higher number. Task 1 Indirect costs (25%) are \$85,147 (not \$101,383). Task 2 indirect costs are \$37,982 (not \$45,225). Task 2 Boyer \$297 per day x 15 days x 3 summers = \$13,365 (not \$27,585). Task 3 Thompson \$52.16 x 240 hrs = \$12,518 (not \$13,155). Parchaso \$37.02 x 320 hrs = \$11,846 (not \$12,338). Task 4 Kimmerer \$7,800 x 100% time for 3 mo = \$70,200 (not \$73,769). No hours are provided for Marvin-DiPasquale. If salaries include raises, this is not noted. Second, there is a lack of justification for many of the large-ticket items. How many people will attend the conferences (\$21,000 for airfare and conference fees, excluding hotel and per diem)? How many trips will be made, how many miles (\$11,000 or mileage for 32,083 miles is requested)? What is the breakdown on the modeling computer and software (\$53,750)? What are some of the bigger-ticket items to be purchased under lab and field supplies (\$53,850)? Is boat use charged by the hour (\$14,400)? If so, estimate hours.</p>
Rating	fair

Overall

Provide a brief explanation of your summary rating.

Comments	<p>Overall, I rate this proposal as Very Good. The results will provide a useful model, incorporating vegetation, fish, invertebrates and contaminants, for decision-makers concerning salt intrusion to the Delta. While perhaps not necessary information today, salt intrusion is likely to occur at some time in the future. This proposal has some deficiencies that can be addressed.</p>
Rating	very good